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these measures completed for the whole area assigned to your observatory within as few years as possible. We have no information how long photographic plates will last. The reductions have been shown to be simple.

(3) When opportunity offers, compare plates by the above method so as to see that no serious error has crept in accidentally. Ordinarily the plates are good enough if they can be measured.

On the Need and Usefulness of Co-operation in Meridian Observation. By Truman Henry Safford.

The following paper grew out of my own experiences. I had for a good while employed my rather scanty leisure for observation in preparing a catalogue of polar stars which had been or could be employed as standards; had published a portion of the right ascensions reduced to 1885, and was desirous of filling up gaps in observing—time, due to the completion of the original project in certain hours of right ascension.

Originally I intended to obtain trustworthy places of all stars to $7^{m}\cdot 5$ inclusive within 10° of the North Pole, and all other stars which had been regularly used as standard polars. The latter class are usually brighter than $7^{m}\cdot 5$, and are more numerous than one would imagine; but at Oxford fainter stars had been also used as standard polars, and in some cases by Mr. Carrington and others.

The enlargement of the working list which at once presented itself as desirable was one which should give accurate positions of four to six stars to every four square degrees within 10° of the pole. By using Lœwy's method of tying the zero-points of neighbouring plates together, the number just mentioned will give the zero-points for the centres of the plates, and the scales will be determined by the overlapping portions common to two plates.

In some parts of the sky, as every observer well knows, bright stars are much less numerous than elsewhere, so that these portions require the meridian observation of fainter stars. There are, for example, portions of *Orion*, where the Radcliffe Catalogue for 1890, containing all 7th magnitude stars, furnishes six stars or more to the square degree. In the same region Professor Valentiner has observed all stars down to $8^{m}\cdot 0$ between the equator and 8° of south declination; his catalogues will furnish the required number for additional areas, where stars of 7th magnitude are too few. His scale of observation is rather more minute than Mr. Stone's, and his results depend quite uniformly on six observations, part of which are unpublished. Meanwhile the Astronomische Gesellschaft is carrying on its survey, to include all stars to $9^{m}\cdot 0$, and all fainter which have been

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previously observed by certain specified astronomers; and we thus have, for the region of Schoenfeld's Durchmusterung, almost everywhere materials enough for the photographic zero-points.

This is not quite so in the northern hemisphere. The Astronomische Gesellschaft's survey dates (on the average) twenty years back. In order to avoid the necessity of extrapolating the proper motions up to 1900, it will be necessary to re observe a sufficient number of stars.

The consideration of the required accuracy at once relieves us of the fear that meridian observation will become a thing of the past. The probable error of a star's position on a photographic plate is not much more than a tenth of a second in either coordinate, if the necessary precautions are taken; and this is very nearly equal to the probable error which M. Lœwy found, to his regret (*Comptes Rendus*, cxvi. 710: April 4, 1893), for the position of the centre of a plate derived from twenty-six stars in four areas. The large probable error is due to extrapolation, or the assumption of zero proper motion. It would be somewhat diminished by using the stars of the Astronomische Gesellschaft, but still more by re-observation.

The following results may be interesting in this connection. In 1892 I prepared a catalogue of 627 stars in declination for the Mexican Boundary Commission. The positions were of course extrapolated, and all the good catalogues I could find were used. If the star was one of Bradley's, the proper motion given by Professor Auwers was usually accurate enough, and could be tested by intermediate observations. If it was only found in Lalande's or Bessel's zones, before it had been observed in a more deliberate way, it was frequently necessary to discard the star, even when it made an excellent companion (in a pair for the zenith telescope) to a well-known star of Bradley's. The stars actually employed gave probable errors in declination, from $\pm 0''\cdot 1$ up to $\pm 0''\cdot 5$, as calculated rather rigidly, with a fair allowance for unexpected sources of error. But these were of the 6th magnitude or brighter; and the largest probable errors were due to uncertain proper motion.

The Coast and Geodetic Survey sometimes use declinations of similar stars, when probable errors are larger than this, even up to $1''$, because in their work the star can be re-observed, if need be, before the final reduction. This is not usually possible in establishing a boundary.

What, then, is the probable error of a determination by four good observations? I assume that the division errors will be rendered harmless by using the instrument in different positions or shifting the microscopes, or by employing more division lines. The objection that there is too great accumulation of small errors in employing the differential method is one which is rapidly disappearing; and thorough ventilation of observing-rooms and truly central field illumination, with great care on the observer's part, and good instruments, now make much more accurate individual observations possible.

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The probable error of one such need not be more than $\pm 0^{s}\cdot 03$ $\pm 0''\cdot 4$ with a good instrument; and allowing something for constant error, the mean of four observations need not have a greater probable error than $\pm 0^{s}\cdot 02$ $\pm 0''\cdot 25$ if sufficient care is taken.

With six stars to a photographic plate, each determined by two independent instruments with this accuracy, the probable error of the centres would be sensibly less than $\pm 0''\cdot 10$ in either coordinate, and the combination with the older observations would be favourable, and still farther diminish the probable errors.

The dangers arising from the use of the differential method need to be minimised by a large catalogue of secondary fundamental stars, and their determination will be facilitated by the latest improvements in instruments and observing. The errors of division of all the single lines or diameters are rarely determined; but it is possible with many instruments to change the circle on the axis. This has been done at Washington regularly, for a quarter of a century, so that the ordinary *Nautical Almanac* stars are quite freed from this source of error. Flexure is more troublesome with large instruments; but the Repsolds have succeeded in nearly getting rid of it by carefully calculated symmetry in their instruments; and the exchange of object-glass and eyepiece, while it may not eliminate flexure entirely, certainly gives two independent flexures, which probably have contrary signs.

The problem of ascertaining primary star-places by direct comparisons with the Sun, and independent determination of refraction flexure, &c., will certainly afford occupation for a number of astronomers for a good many years. But independent primary catalogues are now more numerous and agree better than formerly; and the efforts now being made will doubtless render them far more available for observatory use.

The fact is that the photographic catalogue plates set the standard of meridian observation higher than any former process of astronomy requires. The zero-points of the plates must be very accurately established. This requires the observation of many small stars, with great precision; this again reacts upon the needful accuracy of the many standard stars which are not of first importance; and this again requires that the fundamental brighter stars, which can be well observed by day, shall be fixed with the last degree of care. An accuracy which does not now meet a pretty high standard is but of little use.

In conclusion, I desire to say that Mr. Gill's suggestion for a convention in 1896 seems a good one. If such a convention can be held it would be a benefit, not only to the astronomers of large observatories, who have ample means, but in this country at least to a good many who are limited in their power of effecting large results by scantiness of means, frequently involving many other duties. Practical astronomy is now taught in a good many of our colleges, and those who learn it occasionally accom-

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plish some scientific work. But the instructors themselves need to practise the art they are teaching; and are gradually acquiring the instruments, and in some small degree the leisure, which is necessary. It would be a great benefit to them if they could be so brought into contact with better observers that they could measure themselves against them. And a congress as suggested by Mr. Gill would powerfully stimulate the progress of accurate observing in this country. If, however, such a congress cannot be organised, it is to be hoped that the subject of distribution of meridian observation and its arrangement to avoid waste of labour will be thoroughly discussed by the masters of that subject.

The *British Association Catalogue*, now a scarce and costly book, has been of much benefit, and has done some injury. Its convenience made it a good working list; but its incompleteness, even as to naked-eye stars, prevented a good many astronomers from observing such as were fainter than those it included, or had been accidentally omitted, though bright enough. Thus it happens that the existing star catalogues do not yet completely meet even the wants of field astronomers using portable instruments.

The problem of comet observations with equatorials will most readily be solved when the stars near the paths of these bodies have been photographed, and their places can be fixed with great accuracy.

The principle which seems to me the best in choosing objects for meridian observation of the stars is to classify them according to magnitude.

1^{m}–$4^{\mathrm{m}}\cdot0$	Struve's primary stars.
$4^{\mathrm{m}}\cdot0$–$6^{\mathrm{m}}\cdot5$	Secondary stars, to be regularly and precisely determined.
$6^{\mathrm{m}}\cdot6$–$7^{\mathrm{m}}\cdot5$	Tertiary stars, to be observed with more regularity than they have been.
$7^{\mathrm{m}}\cdot6$–$8^{\mathrm{m}}\cdot5$ $8^{\mathrm{m}}\cdot6$ and fainter	When necessary for photographic zero-points and other purposes.

Each class should be observed with greater accuracy and more care than the following, because in general more important; and stars of 6th and brighter magnitudes should be the standards for the determination of telescope.

As, however, these are largely opinions based on an experience narrow in certain directions, I venture to suggest the usefulness of fully discussing the instruments, the methods, the organisation, the resulting accuracy, and the attainable and desirable accuracy of meridian work. The evil to be avoided is the continuance of any such ancient routine methods as hinder the attainment of the precision desirable.

Williamstown, Mass.:
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On the Effect of Atmospheric Dispersion on the position of a Star.
By Professor Arthur A. Rambaut, M.A., Sc.D.

In these days of extreme accuracy in astronomical observations any effect, however small, that will influence the results cannot be overlooked. For this reason, my attention having been recently directed to the necessity of increasing the constant of refraction as determined by Bessel when it is desired to apply it to the correction of photographic measures (v. *Astronomische Nachrichten*, No. 3255), I have been at considerable pains to ascertain how far it may be necessary to take into account the character of the light of a star in computing the refraction by which it is affected in the case of visual observations.

In a paper by Stephen Lee, published in the *Phil. Trans.* of the Royal Society as early as 1815, the effect of the difference in colour of two stars has been in a general way pointed out, and some observations made by him on the planet *Mars* are described. His observations were, however, wanting in the precision necessary to such a delicate research, and led to no very definite numerical results.

His paper, however, seems to have attracted some attention at the time, and was translated by Professor Brandes, of Breslau, for Bode's *Astronomisches Jahrbuch* for the year 1819.

But although it is obvious when pointed out that the constant of refraction applicable to any particular star will depend on the character of its spectrum, I think it has been very generally assumed that the effect of a difference in the light of two stars will, under any circumstances likely to occur in practice, be so small as to be quite insensible. It has hitherto in all cases, so far as I am aware, been taken for granted that the Besselian formulæ for the differential refraction fully correct for this effect, and that any uncertainty which may exist as to the exact value of the coefficient of refraction has no effect on the resulting measures, since the differential is, in general, such a small proportion of the total refraction. This would undoubtedly be true were the coefficient of refraction always the same for both stars, for even if there were a doubt, amounting to as much as one-fiftieth of the whole, it would but rarely make itself felt in differential measures.

It is, however, quite otherwise if the coefficient is to be considered different for two stars in close proximity to one another. For if the coefficient for one of the two stars exceeds that for the other by a certain fraction of its whole amount, the outstanding correction, which the Besselian formulæ fail to take account of, is the same fraction, not of the differential, but of the absolute refraction affecting the star, and this, even at moderate zenith distances, may easily in certain cases become comparable with, or largely exceed, the correction for the